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Interactive comment on "Role of oceanic ozone deposition in explaining short-term variability of Arctic surface ozone" *by* Johannes G. M. Barten et al.

Anonymous Referee #1

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The authors revise the ozone dry deposition scheme in WRF Chem (now, 'COAREG'). They perform several WRF Chem simulations of August 2008. First, the authors perform a default simulation. Finding that there needs to be nudging to observed winds, they perform a nudging simulation and a deposition+nudging simulation. The paper would be much stronger (and adhere to its goal of investigating the impact of ozone deposition) if the authors focused on the comparison between the nudging simulation and the deposition+nudging simulation, instead of comparing the default and the deposition+nudging simulations. The authors hypothesize that the original ozone deposition scheme in WRF Chem (Wesely) overestimates the magnitude of and underestimates variability in ozone deposition velocity (Vd) over the ocean. They also hypothesize that

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Discussion paper

the magnitude of ozone deposition velocity over snow and ice is overestimated. In general, I think Vd over Arctic land, ice, and ocean are all very uncertain in terms of magnitude and variability. I would like to see this mentioned in the abstract and conclusion. For example, can the authors really say that it's Vd variability over the ocean that driving the improvement in ozone variability when Vd variability over snow and ice is uncertain and likely not represented accurately? The bulk of the paper is about the impact of COAREG on the mean bias of ozone, both in terms of spatial and hourly scales. The authors could do a better job at indicating whether COAREG improves spatial and hourly variability (i.e., be more quantitative). The title, which should be slightly revised (see below), reflects the strength of the paper, which is really in Figure 6 where the authors illustrate that COAREG improves short-term variability in surface ozone at 5/6 sites in the high Arctic and during ASCOS. However, one thing that is unclear is how the authors chose the six sites (out of 25) to highlight in Figure 6. Are these just the sites that COAREG shows a clear improvement at? The paper would strongly benefit from further analysis of short term variability in ozone deposition velocity in COAREG: what's driving the variability in deposition velocity, in particular in periods of better agreement or disagreement with surface ozone? Is it that day-night differences are better captured? Day-to-day variability? Synoptic scale variability? Currently the discussion of Figure 6 seems a bit anecdotal/random. Overall, I recommend major revisions. I think for this paper to have sufficient novelty for publication in ACP the authors need to expand on their analysis of short-term variability at high Arctic sites. The paper is generally well written and clear but can be very wordy and long-winded.

Title: perhaps should be revised to 'Role of oceanic ozone deposition in explaining short-term variability of surface ozone at high-Arctic sites'

The authors say throughout that this is a 'preparatory' study for MOSAiC, but I don't think this does much for the paper. It's not compelling and feels inappropriate to include in a paper. For every field campaign there is a lot that goes into preparations and forecasting, but this doesn't mean it merits publication.

Also, I'm not sure the utility of including CAMS or what I should be taking away from this analysis. Perhaps including CAMS and the default simulation is really for documentation for MOSAiC, but unless the authors can frame the analyses in a more compelling way, then they shouldn't be included here.

The authors are missing ozone flux and deposition velocity constraints from Toolik, Alaska (Van Dam et al. 2016 10.1002/2015JD023914). Please compare how WRF Chem performs. This may signal as to whether terrestrial Vd also needs to be adjusted.

Authors need to revise their use of the term 'background': their usage is incorrect throughout the paper. See Jaffe et al. 2018 https://doi.org/10.1525/elementa.309 âĂÍThe authors need to more clearly what COAREG is/does. Which variables does it ingest from WRF? What parameters or sub-parameterizations are used?

Specific comments Line 5-6: with respect to 'is also overestimated': the authors haven't yet provided an indication of whether ozone deposition to the Arctic Ocean should be over or underestimated, only that it shouldn't be constant. Given that this overestimate is discussed through the rest of the abstract, please give your hypothesis as to why it is overestimated here.

Line 9: I don't know what MOSAiC is

Line 16: I don't know what ASCOS is

Line 30: 'can be' is a bit of a stretch here: these observations haven't even been made.

Line 39: Observations of background ozone are not possible

Line 49: Is Hardacre et al. 2015 the correct reference here?

Line 58: reference for Vd,O3 up to 2 cm/s?

Line 59: Hardacre et al. 2015 should be cited here as well

Line 78: 'the mechanistic representation in Pound et al. (2019)' instead of just 'this mechanistic representation'

Line 76-80: I'm not sure how this is a 'for instance' of an important feedback mechanism

Line 86: do the authors mean 'evaluating with monthly mean . . . observations'?

Line 89-91: not sure what why sub-monthly concentrations will help constrain the "background" concentration. . . please elaborate

Line 91-92: I think the authors need to make a stronger argument that simulated ozone deposition evaluation relies on evaluation of high frequency temporal variability O3 observations

Line 96: reference for iodide controlling longer term changes in Vd?

Figure 1: I don't see a drifting path for the ASCOS campaign. Can you make the line more bold? Can the authors use different colors for the sites that show whether they are high arctic vs. terrestrial vs. remote sites?

Line 155: please address the comment from Ashok Luhar; if the equations are not documented in previous work, please document them here (in particular how equations or parameters are altered for ozone deposition). It's unclear what COAREG is/does.

Line 170: clarify whether only O3 deposition follows COAREG in your simulations: what about other species (you say you are motivated to use this scheme because it provides consistency for all compounds)?

Line 176: please explicitly say what MacDonald et al. 2014 does. Otherwise your reader does not know how to compare the MacDonald + Sherwen datasets

Line 177: is there independent evidence that I-_aq should be higher (and more like Sherwen) than MacDonald? Otherwise the authors need to say that this study assumes higher I-_aq for the purposes of their investigation and that the I-_aq values are highly uncertain (I hope this is something you plan to constrain in the upcoming field campaign)

Line 179: to my understanding, other studies do consider DOM, but they find the effect to be low. Please clarify this here and in the introduction. generally, the discussion of other compounds in seawater could be more consistent throughout the text. I didn't find the sensitivity analyses in the discussion necessary given the lack of details provided.

Line 181: this was recently summarized in Clifton et al. Reviews of Geophysics 2020. I think current understanding is that the POSITIVE fluxes are due to chemistry. I think you should clarify here, and perhaps include the range of observed ozone deposition velocities over snow that Clifton provides

Line 188: please clarify what happens in the WRF-Chem Wesely scheme. Is it exactly the Wesely scheme or some derivative?

Line 195-205: references for diurnal cycle controls on ozone over high arctic vs. terrestrial vs. remote sites?

Line 215: satellite observations of what?

Line 230: I think it's important to show I-_aq concentrations at least in the supplemental because this is an important assumption of your study, and it's not obvious to the reader where concentrations would be high vs. low

Line 231 - new paragraph should start at "Figure 3c shows . . ."

Line 231: This seems like the first time we are hearing about Ra aside from the very quick general definition. Maybe more introduction to this term in the intro is needed (i.e., what does it depend on?) Also, does Ra change from Wesely to COAREG?

Line 240: Have you isolated that temporal variability in ozone deposition velocity is +/-20% just due to waterside turbulent transport? Also, how much is the temporal variability in ozone deposition velocity in the default scheme? It could be 20% as well. So, saying variability from COAREG is +/-20% is not very compelling.

Figure 3: First, I don't think this figure is colorblind friendly. Second, I understand

that the authors want to use different colormaps because the ranges of the values are different, but the purple in both is confusing. What about just two different single-hue color bars? Third, is the point of (c) to show differences in variability or magnitude? I think the former, since the magnitude differences are shown by (a) and (b). Would recommend having two difference y-axes for default and COAREG on (c) so one can see differences in variability more. It would also be helpful to have windspeed on this plot, since the authors talk about changes in Vd with wind speed in the text.

Table 1: Why are there slightly differences in terrestrial Vd between default and COAREG? Please compare COAREG to the nudged simulation. . .

Section 3.2: This section needs revising. It jumps around between talking about how COAREG changes things vs. spatial variability generally, and I'm not sure what I should be 'taking away'. What needs to be clear is how COAREG improves the simulation of monthly mean ozone spatial variability.

Line 252: The authors can be more definitive here. Also, what are the authors getting at? There are clear changes in Vd and the budget. . . Perhaps the authors mean that the differences across simulations are not reflected in the site-level monthly mean evaluation. Clarity needed.

Line 254: This is not a complete sentence. Also, what am I supposed to contrast?

Line 273-9: I'm not quite sure what we are learning from the CAMS reanalysis, and I find it particularly confusing to have it discussed in each section. In the least, I suggest all discussion of CAMS be moved to a separate section at the end. I don't know enough about CAMS to be able to interpret the meaning or cause of differences.

Figure 4: It really does not make sense to me to show the default simulation in Figure 4. The nudged simulation should be shown here to illustrate differences due to ozone deposition, the point of the paper. I think the authors need to present some statistics as to how the different model simulations capture spatial variability in monthly mean

ozone.

Figure 5: I find it strange that the authors are just focusing on mean bias and MAE here, when they say in the text that they want to look at short term variability. How does COAREG improve variability? It would be helpful for the reader if the authors included some information as to how the diurnal cycle of Vd changes with COAREG. Is the important thing the diurnal cycle or day to day variability?

Line 311-3: 'to a lesser extent' than what? Generally, closing with this statement makes me question the authors' use of a regional model here. Is this the authors' intention? I suggest revising.

Section 3.4: This section could be more quantitative. It's unclear why the authors chose to discuss some features of the intercomparison and not others.

Line 318: How do the authors select the sites used in Figure 4? Do they just choose the ones at which that the COAREG scheme performs best? This is concerning, given that the title and conclusions majorly depend on Figure 4.

Line 327: new paragraph starting at 'At Summit,'

Line 334: cut 'Interestingly' and start new paragraph here.

Line 376-8: exactly why the nudged simulation should be the 'default' simulation here

Line 390: say why: because there are no observations, right?

Line 443: spelling error

Line 445: new paragraph here

Line 459-66: please cut this paragraph of 'next steps' in an already lengthy discussion; it's not really appropriate for a paper

Line 470: for all trace gases or just for ozone here?

Line 490-3: this is similar to the finding of Clifton et al. 2020 10.1029/2020JD032398 that when the ozone lifetime is long ozone is very sensitive to small changes in a small deposition velocity

Line 505: why is this revision needed at the global scale? Why is short term variability in ozone at high arctic sites important for ozone globally?

Line 508: what is the 'fate of the arctic O3 budget'?

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